

Dana (Sam'l. L.)

LEAD PIPE,

ITS DANGER;

A

REJOINDER TO THE REPLY

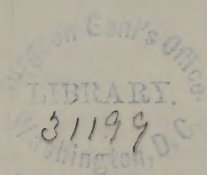
OF

PROFESSOR HORSFORD

TO THE

ARGUMENT ~~IN THE~~ APPENDIX ~~TO~~ TANQUEREL.

By SAMUEL L. DANA.



LOWELL:

DANIEL BIXBY AND COMPANY.

1848.

LEAD PIPE:

ITS DANGER.

TO PROFESSOR HORSFORD.

SIR: — In your final Report “to the Water Commissioners of the City of Boston,” (*City Document, No. 32, Appendix, p. 39,*) you state that “the argument against the employment of leaden service pipes for Cochituate water has been presented to the public in a strong light” by myself. Certain reasons are then given, which “induce you to present the argument in *favor* of employing lead for the purpose above mentioned — in the form of a reply, if not in the order, yet in general to the substance of the argument above alluded to.”

I shall find no fault with any order you may please to adopt, but as the argument *against* was stated and defended in a plain and direct manner, I cannot but regret that you have not given a plain and direct reply in *favor* of lead pipes. As it is, I beg leave to offer a few remarks on the ten small parcels into which you have divided your “Reply.” If my observations should assume a somewhat discursive form, it will be because your rambling argument forbids a condensed rejoinder.

“I. *Lead is always present in the tissues and blood of men not only, but in the tissues and blood of domestic animals.*”

I would remind you that this forms no part of my article on lead pipe. I do not object to your deducing as a principle, that which is so evidently stated in the paper of the Appendix to which you refer, that “small amounts of lead may be present in the body without injury to health,” but I must insist that you shall state from this same paper, that beyond this small amount, disease and death are the consequence of the increase. That was no small part of the object of the article from which you have deduced your “important principle.”

“II. *Numerous wells and springs have consumed lead pipe.*”

On this point we agree. You are fortunately in possession of more “startling facts than any here noticed” by myself, and you agree also that “in consequence of the lead in water derived from supply tubes, there have occurred frequent and frightful cases of illness, and occasional deaths.” But concurring as we perfectly do on this point, I can see neither the propriety or force of your stating the principle above in an “argument in favor of lead,” without attempting to combat or disprove it.

I appeal to your candor whether I have not demonstrated, equally as clearly as the above, that several rivers have acted on and "consumed" lead pipes?

"III. A prominent action of well water upon lead is ascribed to the nitrates."

Here also our opinions coincide. You give credit to some one for "an observation of more substantial value for practical guidance than almost any contribution that has been made to this subject." I cannot but express, therefore, my surprise at your perfect silence respecting the author of the contribution to this point, years gone by, in your third Report, in which you have stated a principle derived from that person without one word of allusion to the source; leaving the impression, that the whole originated with yourself. I regret to see so early, an indication in you of a disposition to appropriate as your own, whatever of merit may be due to the original observation of another.

So far then as the presence of nitrates is an argument *against*, that cannot be urged as an argument in *favor*, of lead pipe in waters like the Cochituate and Croton — in the first of which Prof. Silliman, Jr. detected nitrates, and of the last, he observes, in his Report to the Water Commissioners, 1845, "it contains a *large* amount of crenic and *nitric* acids."

As you have now adopted a classification of waters, founded on the principle pointed out by the author of the observation on nitrates, you have done wisely in attempting to show how an assumed small portion of nitrates in lake and river water will be annihilated by the organic matter there present, leaving us to regret, if your theory was practically true, that Cochituate water is not more impure than actual analysis has proved. Large as is its amount of organic matter, the analysis above referred to shows that a portion of nitrates is also present.

Your table of the effects of adding saltpetre to Cochituate water is full of instruction. It shows not only that its solvent power on lead is increased, but that it is more enduring than that of pure Cochituate water, and that even the last, in this series of experiments, has not *ceased* its action after the second day, as is pretended in your first, and again and again repeated in your second and third Reports; but here, it acts as much on the fourth day as it did on the second day in your first Report, and even now continues to act as long as the sixth day. I shall take leave to use this fact in the course of this discussion. I trust, Sir, that you have examined the nature of the solutions obtained by saltpetre, and have satisfied yourself that nitrate of lead was there present; for as you have not stated that *air was excluded in these experiments*, or is it even probable that it was, the blue coat of *suboxide*, which you affirm is the only coat formed in all your experiments, would not be dissolved even by saltpetre. One or both of the other red or white coats which you have observed must then have been formed, but this is utterly at variance with your repeated statements, that in Cochituate water the blue coat only forms. But some coat, as shown by the experiment, has been solved, and this coat was probably that which all other observers have noticed, a hydrated oxide and carbonate of lead. Nitrate of lead must then have existed in the solution, IF your deliberate statement should ever be proved true, "*that when a solution of saltpetre is poured on carbonate and hydrate of lead, a NITRATE OF LEAD, AN EMINENTLY SOLUBLE SALT, IS FORMED!!!*" (*City Doc. No. 32, p. 35.*) Sir, this is a chemical decomposition of such monstrosity, that I cannot believe it, even had I the credulity of certain Water Commissioners, and I wait patiently for your proof.

"IV. *Tanquerel, deeply skilled in all the phases of lead diseases, did not 'recognise the minutest shades' of these maladies among the citizens of Paris, who drank water supplied through leaden tubes.*"

Perhaps it may be as well to state to those who have not seen Tanquerel on Lead Diseases, that he states simply, that "he had not learned that *colic* was thus produced." The inference, that "he would have recognised the minutest shades," &c. &c., is mine, and for the argument I put it in this broad form in my Appendix to Tanquerel's work. I have shown, that even with this wide allowance, covering all lead maladies, the fact may be accounted for in two modes, by the quality and by the quantity of water used by a Parisian from lead pipes. The argument rested almost wholly on the quality. To this not a shadow of reply has been even attempted. To show how little I relied on the quantity, I distinctly stated "that even allowing for the largest amount" used in other European cities, the qualitative part of my argument would hold good. Now after you have *doubled* the amount of water which I showed was used by a Parisian, you dismiss the argument from quantity by a mere supposition, "that not the quantity drank, but that used for other general purposes is less," and then account for the absence of disease in the following happy and logical manner. "The less the quantity of water used, the longer will be its exposure in the pipe, and the *more lead will there be in solution.*" That is, the more lead one drinks, the less liable is he to lead disease. This is certainly an argument in favor of lead pipe, and had it been earlier urged, it would, perhaps, have saved much valuable time and useless experiment. However, I am much obliged by your distinct avowal, that the longer water is exposed in lead, the more of that metal will be in solution. How powerfully the conviction of the ceaseless corrosion of lead pipe, even by water of the highest protective quality, stands out in these few words! Like the fact you have above demonstrated in relation to a continued action of Cochituate water, I shall take leave to use it, as your well considered admission of a law which cannot be denied.

Here is river water in which no traces of nitrates are recorded, in which the power of forming a protective coat, you cannot but admit, is of the highest order, yet stated by yourself to dissolve more and more lead, the longer it is exposed to that metal. How does this agree with the doctrine so strongly urged upon your experimentation, that in river water action wholly ceases after the first few days? But of this by and by.

"V. *Croton water supplied through leaden tubes is believed to be injurious to health upon the following testimony.*"

"Dr. Dana received in 1846 for chemical examination some Croton water drawn directly from the street main through a leaden pipe into the house for family use." "This statement," you continue, "was difficult to comprehend." It may be so, but I am sure that it is not the fault of the statement. That is sufficiently explicit, or I know not my mother tongue; I suppose, however, it was not so incomprehensible that I should thus receive water, as that I had discovered lead in it. That it was which, to use your own expression, "startled" you.

This part of the testimony should have been stated. You have entirely omitted it, and thus left parties unacquainted with this discussion, except from this part of your Report, to infer that I had detected lead in Croton water.

The "testimony" on which I founded my belief that Croton water lead transmitted was injurious, rested, 1st, on detection by myself of lead

in it; 2dly, on similar facts, established by Dr. Chilton; 3dly, on the deep erosion of Croton lead pipes by use. This is the direct testimony. It was supported, 4thly, by the letter from a lady, which you have quoted, showing the fact of the presence of lead by the deleterious influence of such water on herself; and by similar facts stated by Dr. Chilton. The whole was introduced not to *prove disease*, but to *disprove* your position, that lead ceased to be acted on.

Once more, in my letter to Dr. Warren, published in the Atlas of March 11, 1848, a part of the ground of evidence above stated, was introduced to prove the "similarity of the action of lake and river water on lead." I stated then and there, "during the year 1846, I was requested by a medical friend to examine some Croton water drawn directly from the street main through a leaden pipe into the house for family use." This is the sentence which you have substantially quoted above.

Mixing up my main argument against your principle of cessation of action, with an illustrative fact, stated in my letter to Dr. W., for a different purpose, you profess to have been unable to comprehend the last, and even intimate you are "startled" thereby.

Your amazement seems, however, to be of much later date than your knowledge of the fact. In the several pleasant interviews I have since had with you, no explanation has been desired, or allusion made, to any surprise about this statement. In the many friendly notes which I have received from you since March 11, no allusion is made to such feelings, or is such an idea hinted at, in letters to another of your friends speaking of my published letter. In your third Report, dated May 2d, you simply state the facts derived from this same letter, that "Dr. Dana has found lead in Croton water, drawn from the street mains," as having "great significance," without expressing the least doubt or difficulty relating to the water being directly drawn. The statement was then evidently comprehended and understood, as it was made in good faith. And now, how do you meet the argument you have thus mutilated? By first doubting whether I have received water drawn directly, and then, when assured by the party who sent it, that I had, assuming in order to account for a fact, which you have omitted to state, that I had applied a label of a part to the whole of the water which I had received. You addressed several queries to the gentleman who transmitted the water, and among others this: — "How long had the water drawn for examination been standing in the pipe?" Answer — "I filled two pint bottles, one with water that had *remained in the pipe twenty-four hours*, the other after the *hydrant had run some twenty minutes*. Unfortunately, in sending the bottles, the labels were lost, and the contents were probably put together and then examined." You then addressed a note to me, June 7, wishing to know whether I had examined "both bottles, as they were drawn under unequal exposure." Now I would ask you what was my immediate reply? In the face of the direct assertion of the gentleman in answer to your query above, that the labels were lost, in face of minutes from my laboratory notes, sent to you detailing substantially all that I did, and why I did it with these bottles of water, and in face of my note, stating that the details above referred to were "sufficient to allow you to infer a high probability in the one bottle of the presence of lead, and such a degree of certainty in the other," that, as I then added substantially, even a reduction of lead to the metallic state would not be more satisfactory to you; and after thus having my statement that both bottles contained lead, you gratuitously assume, that "the label of the single bottle that contained the water drawn directly from the main, had probably been the only one preserved!"

Your correspondent, in his answer above, has given me the first information I had received, that the *bottles* were ever labelled. As I stated in my note to you, "the *bottles* came to me without other intimation than that they contained Croton water drawn from lead pipes in New York."

But you say farther, that my answer (on June 8, to your note of the 7th) "explained the difficulty," and that the conviction that the label you assume, was applied to both bottles, not only "startled" you, but led me to believe that Croton water drawn directly from the mains could contain lead. I believed that without a label, I still believe it, now that you have supplied one, for the evidence that both bottles contained lead, was equally decisive. I relied but on one, for reasons stated to you.

There was no "cursory" examination, as you have stated. The lead was equally precipitated from both, and judging by the mode of estimating quantities, so familiar to yourself, the quantity in each was alike; but in the one case the precipitate was "thoroughly examined," in the other neglected. All this was familiar to you when you penned the statement that I "had examined but one of the bottles thoroughly; the other yielded, on a cursory application of tests, only the usual reactions of water containing a considerable portion of organic matter!" If the phenomena with sulphuretted hydrogen, with this water, as detailed in my letter to you, are those common to all water containing much organic matter, then are your own results vitiated, because not separating and weighing your lead precipitates by sulphuretted hydrogen, you have determined their quantity only by the bulk presented to the eye. But of this hereafter.

I must protest against your partial statement of my laboratory notes; for in so doing, you treat me with great injustice.

Such a statement, if left unnoticed, places me in a false light before the parties sending, and the medical friend from whom I received the water, and with whom alone I corresponded on this matter. In one of the several letters which passed on the subject of this water, before I analyzed it, or even opened the package which contained it, I was made acquainted with the wishes of the parties respecting "the two pint bottles," and when, finally, I communicated my results to my friend, I stated, as I now find by referring to the draft of my letter dated Nov. 10, 1846, that I had "minutely examined the two bottles." That letter was communicated to the parties who sent the water, and if it still exists, after its various passages among the friends of the New York parties, in Boston and Philadelphia, it will show whether this statement is not there repeated.

My own self-respect leads me to deny the correctness of your statement, though I may thus leave you in a situation to be "startled." But why should you be thus affected? Is there any more improbability, that water drawn in New York directly from the mains through lead should contain that metal, than that the water at the Giessen laboratory, drawn directly from its reservoir through a lead pipe, should be thus impregnated during its passage? Were you never "startled" when you heard that new comers, among the pupils of that celebrated school, were cautioned not to use water thus drawn in experimentation, because its lead would vitiate their results? Have you ever determined how long water must be exposed in a lead aqueduct before it is contaminated by its presence?

And now, having explained satisfactorily to yourself, your difficulty in "comprehending the statement of" this portion of the argument, what was your next step? To attempt, either to verify or refute my results, by examining the water drawn from the identical source, after thirty-six

hours' exposure. If you did not find lead then, of course it would be quite improbable that I could detect it after twenty-four hours, and impossible if drawn directly. The result of your assay was, that now, for the first time, after many trials, you detected lead in Croton water after thirty-six hours' repose in a lead aqueduct, and might have detected it, you observe, even after fourteen hours' repose, only in *less* quantity. I have already alluded to your mode of determining quantities. *I have not*, and I have yet to meet a chemist skilled in laboratory practice, *who has*, any confidence in your mode. It is as little worthy of reliance as would be that which, determining accurately by weight the lead dissolved by a given water in a given time, should then estimate the amount of that dissolved in a greater or less period, by the proportionate times of exposure. Alas! I had forgotten that this mode also is sanctioned by yourself. I therefore shall not take notice of any quantity you have thus pretended to determine.

And now let me ask, what your trials to find lead in Croton water have to do, in disproving the proposition "that Croton water supplied through leaden pipes is believed to be injurious to health," upon the testimony I offered? All that might have been reviewed and commented on, without your detecting lead. Your success has but added new proof of the continued and ceaseless action of river water on lead, and showed, that the identical pipe, from which my sample of water was drawn eighteen months before, was still undergoing solution.

This fact in my testimony, having been thus confirmed by yourself, what is your next effort to set that testimony aside? I said, "the evidence derived from chemical analysis was supported by the fact, that at least one member of the family using water from the source whence was drawn that analyzed by myself, was affected with lead malady." (*Appendix to Tanquerel*, p. 382.) I called my witness. You have attempted to discredit her, as a nervous woman, apt to imagine disease, and not satisfied with calling especial attention to her excellent, but succinct letter, republish that, *italicising* certain portions for reasons which you do not choose to express, and intimate that all her sufferings, paralysis excepted, were the simple effects of Croton water on her peculiarly susceptible constitution! Sir, if any portion of that lady's letter deserved to be *italicised*, I appeal to your sense of justice, whether honor and candor would not have selected this—"I have been affected in the same way, both in town and country, by rain water which had never come in contact with iron, but which passed through leaden pipes." She has verified your profound remark, that "*all* waters in some sensitive constitutions are either poisons or antidotes." Croton water lead transmitted was to her a poison, while pump or hard water was its antidote.

Finally, you have stated only a part of the testimony of Dr. Chilton to the point under consideration. You omit entirely his evidence relating to disease as given in the Report of the consulting physicians, to which I referred my reader, and impugn his chemical researches, because they "lack the attribute of quantity!" Do you mean that his observations were limited to one or two cases as your phraseology indicates, when his letter speaks of "several instances," or do you object to his testimony because the actual weight of the lead is not stated? Had Dr. C. the happy tact of determining the weight of precipitates by the color they present to the eye, we might perhaps censure him for not stating in good English grains that amount, without mystifying his readers with hundredths of thousandths of French grammes.

I. *The protection arising from carbonate of lime and magnesia is led to be diminished by free carbonic acid in water."*

sever This belief is founded on very high authority; you attempt to show how carbonic acid protects lead by driving out the oxygen from water, and take no note of the carbonates. Has the largest amount of carbonic acid yet discovered in Cochituate water, driven off all its oxygen, so that this water no longer can form your blue coat? Why, this argument, were the question answered affirmatively, is set aside by your own declaration, that carbonic acid can "have little value in settling this question; since the measure of carbonic acid present at any time in water is never constant." It is then more likely to destroy your explanation, than to affect my belief.

"VII. *Preference is expressed for tinned copper pipes."*

After what I have said respecting your quantitative determinations, it is not necessary for me to reply to your table of the effect of *two days'* exposure of tinned copper tube. I will merely add, that in your tin experiments, if they possess the attribute of quantity, they "lack" that of quality. What evidence have you, that your precipitate was tin? This is a "significant question" to those who happen to know, that some of your pure experimental tin contained a large per cent. of arsenic, which was by its color mistaken for tin when precipitated by sulphuretted hydrogen. I shall look with impatience for your "detail of the results of your investigation of the subject of aqueduct pipes, as a contribution to science," where I hope these questionable matters will be made clear.

And let me ask you, how you can consider an experiment of *two days* as confirming your previous statement, that no protecting coat is formed? Do you settle that for tin, in two, when perhaps a third day may have shown a protecting coat? Is it not as probable that the action on tin would cease, as it is that lead should have its fits of action and non-action?

Such is the fact, shown by yourself, with respect to lead, ceasing to be acted on at the third, acted on at the fourth, ceasing the fifth, acting on the sixth, and again suffering its period of non-action on the eighth day. If such is the fact relating to lead, it is possible tin may be subject to an intermittent action like lead. Your experimentation lacks the attributes of quantity, quality, and duration.

And then once more, if it is, as you urge, an important principle that "small amounts of lead may be present in the system, without detriment to health," may not the same be urged in favor of tin? Certainly, and with much more reason, the actual solubility of tin is yet questionable; if solved, it is not proved, that any increase beyond the "small amount" has ever occasioned disease or death; whereas, these accidents are common and well known facts with respect to lead.

It seems to me, therefore, allowing that tin is solved and forms no protecting coat, reason dictates that this metal should be preferred to lead. Besides, you have never shown, or has the attempt been made to show, that if, perchance, the outermost film of tin on copper is dissolved, that the substratum of *bronze* which then covers the copper will be acted on. All experience, all careful observation is against such an action, and till you have proved that, beyond a possible doubt, I shall continue to express a preference for tinned copper pipe.

One word more on this point. I do not comprehend why the subject of gutta percha pipes should be mixed up with my preference for tinned

copper. You spend a page about this matter, and if you had *im-*
gutta percha unfit for water pipes, that would be no argument in favor
 lead beyond any other metal, tinned copper for instance.

“VIII. *Several reasons for distrusting lead as a material for service pipes are given.*”

Not one of which have you shown to be unsound. They do not tally with your reading and observation. That is all. Let us see how this matter stands. You say you have not been able to devise any experimentation, demonstrating the action of *oxide of iron* as “a stream of oxygen to hasten the corrosion and solution of lead pipe.” Nor have I. That oxide of iron should act so, would be attempted to be proved only by one who believed in the decomposition of nitrate of potash by oxide of lead. Both are alike impossible. I have never assigned such a reason for distrusting lead. That peroxide of iron may and does give up its oxygen to tin and to lead, and to several other metals, has been proved by Napier, and in our conversation on this subject last summer I referred you to two publications where his experiments are recorded.

Besides the aqueduct case, which is quoted from the transactions of the British Association, cannot be, and has not been explained on other action than that of peroxide of iron. It is a great practical proof of the truth of Napier’s experimentation, with persalts of iron. You have also called in question the formation of free sulphuric acid by the decomposition of sulphate of iron by absorbing oxygen.

I know of no chemical fact better or longer established than this, and the slightest reflection will show, even without the aid of a formula, what must be the result of that decomposition of a solution of copperas by exposure to air or oxygen, when tetrasulphate of iron precipitates, as it uniformly does in this case.

And as for the action of “*alkaline chlorides*, in not requiring the presence of air in water in order to the solution of lead,” I have nowhere so said, or alluded to such a relation between air, alkaline chlorides, and lead solution. “Supposing a compound of lead and chlorine to be about forming, no common air or free oxygen is necessary, as is the case when vegetable or other acids form their salts. Chlorides would form in lake or other water, were all its common air or free oxygen withdrawn.” That is my statement. You object also to “the propriety of my reasoning from the observed action of rain to the probable action of Cochituate water.” Till you have proved the want of “resemblance of Cochituate to rain water,” where is the impropriety? Is it more improper, than for you to reason from the action of Croton to that of Cochituate water? Why, Sir, your objection strikes at the root of the argument for the safety of lead pipe in Boston, drawn from its presumed safety in other cities.

There remains but one more of my several reasons for distrusting lead pipe which you have enumerated, — the solubility of suboxide of lead. Grant that it is not soluble, is your argument in favor of lead pipe strengthened thereby? Certainly not, till you have proved, that the common oxide would not result from the action of carbonic and other organic acids on the suboxide. This higher oxide would then be dissolved by water, or form soluble compounds with these acids, and hence, as has been often shown, if carbonic acid is present in water, a solution of lead results. But I defer farther remarks on all these chemical questions, till the chemical part of your researches shall come under review.

4. *The action upon leaden pipe is believed not to cease."*

This is the main point in my argument against lead pipe. It runs through the whole, first stated and never lost sight of. You have taken it up as if a point of little importance, and have replied to it in a manner equally singular and inconclusive, but not more inconclusive than irrelevant. And now let us for a moment pause and ponder on the importance of establishing the truth of the above ninth proposition.

The report of the Consulting Physicians of Boston on the subject of a material for distributing pipe for Cochituate water, deciding nothing positively relating to choice in this matter, yet is not unfavorable to lead. The opinion expressed of it is based upon the reported result of the effect of lead aqueduct in several large cities, London, New York, Philadelphia, Boston. "No practical evil of a *general* nature is known to that board to have resulted in these places from the employment of lead." Yet this happy result is "seemingly discrepant with the general facts, universally admitted by the common experience of the world, respecting the deleterious action of lead on the animal system; and the solubility of lead in some cases in natural waters."

The consulting physicians requested the assistance of yourself and Dr. Hayes, "to put to a suitable test the action of the waters of Schuylkill and Croton rivers, Jamaica pond and Cochituate lake upon metallic lead. The board then state the following: 'An interesting result deducible from Prof. Horsford's experiments is, that the solvent action of these natural waters upon a given surface of lead, diminishes from day to day, and at length wholly disappears.'" This seemed to reconcile the discrepancy. It was a statement wholly at variance with the results I had long been conversant with. If true, it was a result from your early labors, whose importance in every point of view, could not be expected to be eclipsed by a long life crowded with useful discoveries in your favorite science. And here let us be sure that their inference from your experiments was well founded. In the first report which you have made on this subject, is the following relating to the action of the above waters on lead.

"4. With each succeeding day the action is less and less, and in a few days ceases altogether with the Cochituate, Croton, and Fairmount." The Cochituate acted not after the second day. Again, speaking of the amount of lead in water which had stood in a pipe two months and a half, you remark, "It is probable that the action terminated as in the case of the bars," (in your experiments,) "the pipe having become coated with a compound of lead insoluble in and impervious to the action of the water." Again, a pipe being filled and emptied daily for several days with Cochituate water, that "will cease to act upon the lead altogether."

This ceasing of action was repeated in your second and third Reports. The physicians cannot be accused of overstating your results, or for having stated as a deducible result, that which you have thus repeatedly declared as a well settled fact. This position was put forth by you upon the slight evidence of a few experiments on five square inches, or less, of lead surface, subjected a few days to half a gill of water in an open glass tube, exposed to the air, light and gas of your laboratory! Are these the conditions of lead pipe in actual aqueduct service?

"These experiments may explain the cause why so many persons drink the water from leaden pipes without detriment, and why the waters from Jamaica Pond and other aqueducts have so often failed to furnish the evidence of the presence of lead, when submitted to the test of chemical experiment." (*Report of the Consult. Phys. p. 9.*)

I have, I trust, represented correctly your position, and the use be you at once permitted to be made of your statement. You state a fact. The Board make use of that, without any objection on your part as having been drawn from unfinished experiments. You consider the fact established, and the use of it legitimate. Sir, allow me to say, that so important a principle was never before adopted upon such inconclusive evidence as you have given of its truth. It is such evidence as has never before been considered so unimpeachable, as to be at once applicable to the removal of a great practical objection to the use of lead pipe.

Your proposition was opposed to all experience. It was not to be disproved by counter experimentation, for so far as that had been done, the results of a chemist, second to no man in any country for accurate experiment and acute observation, had been laid before the Board of Consulting Physicians. Besides, I had stated my objections to all such experiments as inconclusive on certain grounds, (*Letter to Dr. Warren*.) objections sufficient to induce at least caution in applying the results of a few laboratory experiments on this subject to waters wholly different from any heretofore used for the supply of large cities.

The principle you had announced, that water ceased to act on lead after a few days, was therefore to be tested by appealing to the actual results of aqueduct experience. The question relating to health, derived support from this same source, experience, and was very favorably received by the public.

I have shown that your position is not sustained, by a mass of testimony, drawn from this source, in every variety of water. This testimony depended on corrosion of the aqueduct pipes, as a part of the evidence. It is the only part you notice. No attempt is made to disprove the fact of corrosion by river water, or to set aside the evidence from other sources, except perhaps that from disease, as corroborative proof of continued action. But you must be aware, that evidence introduced merely to corroborate a fact already well established on other grounds, affects not that fact, whether the corroborating evidence is admitted or disproved. You make no attempt to set aside this other ground of evidence. You cannot but feel, that attempt would prove fruitless — and why? Because yourself it is, who all along have furnished evidence of the fallacy of the ground on which you professed to stand. Your very first Report, dated March 9, 1848, shows your want of confidence in the results of your own experimentation, when they are to be practically applied. Each subsequent Report testifies to the same fact.

Your experimental result showed that Cochituate water ceased to act on lead at the end of two days. Instead of abiding by this result, you direct, in practice, that the pipes should be filled for twenty-four hours, and then emptied, for three successive days; and thereafter, for the fourteen following days, from the first morning's running, a gallon from the smaller, and two gallons from the larger, to be thrown away. Does this look as if action had ceased in two days, as in the experiment, or even in a few days, in the common use of that phraseology?

Has any thing but the evidence I have offered, or similar evidence observed by yourself, that, practically applied, your position cannot be for a moment maintained, induced you to increase this precautionary measure to four weeks? (See *Final Report*, p. 49.) Why is this, — if action ceases as early as your little glass tube trials led others officially to report, and as an inference only from them, that action ceases in a few days?

Besides, appended to this very report, in which this remarkable principle is brought to light, you adduce the letter of Prof. Graham, of

who bears his testimony, if not directly, yet by legitimate inference against you. "No evil," he says, "is experienced from lead pipes or cisterns, yet the water must remain in these, (the cisterns) for several days." What then? Not that lead is not thus dissolved, or produces no disease; for, continues Prof. G., "if Boston should have the advantage of a constant, instead of an intermitted supply, the cisterns will be unnecessary. The constant supply has lately been considered highly necessary on sanatory grounds." Can other inference be drawn from this, than that lead is dissolved in these cisterns, that disease follows in some cases the use of the water, that on "sanatory grounds" the cisterns should be dispensed with, and that is so important, in his view, to the Bostonians, that "the constant supply ought to be insisted upon in the outset?"

And why is it that almost at the very moment you are experimenting in the laboratory to show that lead ceases its action after a few days, we hear of reports to learned societies in London, that experiments to protect lead from the continued erosion of water, by the galvanic action of other metals, have all signally failed? And, why should such experiments have been made in London within two years, if it was not a fact that lead is constantly eroded, and that on "sanatory grounds," therefore, attempts are making to counteract this action.

As for the evidence of the Consulting Engineer of the Boston Water Works, that has been already elsewhere shown to be irreconcilable with your experiments, I merely allude to this here, that you may not fall back upon the fact that he refers only to well water. Sir, your Reports, in which this cessation of action is announced, touch the general question of the action of all water. Happy would it have been, if you had confined your remarks to the single question submitted to your experimentation. You go out of your way to draw facts from lead pipes in actual use in wells, and when you find $1\frac{1}{2}$ grains of metallic lead per gallon, which two of the Water Commissioners call a "*trace!*" in pipes several years in use, instead of meeting this fact, so directly opposed to your principle, it is explained away by the presence of sulphate of lime in large quantity, that "being the single distinguishing attribute of this water thus far ascertained." (See your *Third Report*.)

What kind of chemical examination must that have been, which should allow *nitrates* to escape your notice then, by which, on mere supposition of their possible, without any experiment to determine their actual presence, you now allow the commissioners to explain the fact of the continued erosion of lead? It is much to be regretted, that having almost under the windows of your laboratory, four wells which you report did not act on lead, and one which did, that on your adopting an explanation of the unequal action of different well waters, by the "*unequal measure of nitrates*," you have not stated the exact amount of these salts present in the one, and their absence or inappreciable quantity, in the remaining four. I have a right to infer that no such examination has been attempted, from the circumstances that the Water Commissioners infer the presence of nitrates in the one well from the fact that it is shallow, and the water for your experiment from another well "was drawn from a depth below the clay, which is impervious to rain water." On a point of such great importance is it just to leave that to be inferred, which you could have so easily, if true, demonstrated? I will only add here, for your information, that I am now writing over a stratum of hard blue slate, which with mica slate underlays all the central parts of Lowell, a few feet under the surface. Within a few rods of me, a well has within

a short time been sunk into this rocky stratum. All other wells be stopped out except that which wells up from the rock twenty feet from the surface of the ground — and yet this water, collected free from water in this stratum of rock, impervious to rain, contains a large amount of nitrates. I know of no wells in the city which contain more.

No one here would be so foolhardy as to put a lead pipe into this water, yet, marvellous to relate, it has not acted on little bright bars of lead in a glass tube in my laboratory, occasionally adding fresh water, for three months, except to form a blue coat apparently! Have I a right to infer the absence of nitrates, when experiment had demonstrated their abundance? Or can I conscientiously tell my neighbor that he may use a lead pipe with impunity in this well, because my little bright bars of lead only turn blue-gray, and the water in my slight experiment contains no lead? Every well in this rock, situated as is this, has acted on and erodes lead pipe in actual service, thus proving how inadequate such experiments as yours and mine are in this case, to establish the fact of the cessation of action of water on lead.

Your final report furnishes abundant evidence of the baseless foundation on which your principle of cessation of action reposes. I have already alluded to your experiments as having confirmed the facts contended for on my part, that the action of water on leaden pipes does not cease. Action never ceases in *any water*, even in those whose protective power is the greatest. Let me refer you to your own admission respecting the Paris waters, already noticed under your fourth proposition.

The pure Cochituate water, you now show, in a new series of experiments, acts three times as long as in your first trials. You have found lead in Jamaica pond and Croton waters, years after the lead pipes have been in use. What other evidence do your friends require, to prove the fallacy of your pretended principle, when applied to river and pond water?

If, then, all experience has been shown to be against your doctrine; if I have, as you cannot deny, proved it untrue, by the repeated detection of lead in aqueduct water, by very competent chemists, years after the pipe had been laid down; if their testimony, and the results of my own humble efforts in this way, are confirmed, as you now are happy to admit (see *Final Report*,) by your experiments; if to the results of scientific research, I have added, as the Water Commissioners allow, (*City Doc. No. 32*, p. 14,) “specified cases” of lead disease from the use of river water lead transmitted, as corroborative proof of the continued action of water on lead, taught by corroded pipes and scientific research; — then, Sir, I claim to have proved your principle wrong, and I am entitled to say, not as you have made me say in your ninth proposition, that “the action on leaden pipe is *believed* not to cease,” but that it **ACTUALLY DOES NOT CEASE IN LEAD AQUEDUCTS.**

And thus having proved this point, I said, that unless all experience of the effect of small doses of lead was to be disregarded, that disease would follow as a corollary from the point thus established. Hence the last and tenth proposition which you deduce from my argument, is thus stated by you, — “*It is believed unsafe to employ leaden pipe for distributing Cochituate water.*”

Allow me to say, with all becoming respect, that I cannot descend to notice your calculations, on this point, of the quantity of lead which will prove fatal. Your data are founded on your once having detected in “500 c.c.” of Jamaica pond water “0.00002 grammes of lead;” and on an amount of lead in the whole system, deduced from the analysis of

, of a part of the organs of a man who had died from one of the forms of lead disease.

Profoundly elaborate calculations are gone into by you to show, how much of lead water, "warranted not to cut in the eye," of a good quality, such as you drew after thirty-six hours' repose, from the Jamaica pond lead aqueduct, a man must drink, before such beverage, at the rate of a gallon a day, will kill him. Pray Sir, will you enlighten the world with an equally profound calculation on the quantity of lead it will thus take to *induce* any given lead malady? That is the practical question which interests living men. What consolation is it to assure a man, from school-boy calculations, that if he drinks as much lead every day as this ciphering shows, he may live 3465 years, when he knows, that during this long lease of life, he is suffering from the effects of his first few draughts, and has the positive assurance of universal experience, that every draught he drinks adds to his amount of suffering? Is the hope of a long life cheering with this prospect in view? This, Sir, is the practical question. To help you to solve it, I beg leave to remind you of the numerous cases, where the mere *inhalation*, even for a few hours, of the smell of lead paint, has caused serious lead disease; and of those other, but unfortunately not rare cases, where, by water containing infinitely less than the "trace" of lead you found in a certain well, and which the Water Commissioners state "proved injurious to the health of the family," has been caused, in a few days, unmistakable lead disorder.

Let me remind you also of the Tunbridge Well, quoted in my Appendix to Tanquerel. Till you have solved this question, I trust I shall not be thought "yielding to an alarm, which appears to have arisen from an imperfect knowledge of the facts pertaining to the case," if I believe, that it is unsafe to employ lead pipe for distributing Cochituate water, not only because it may finally produce death, but because it will produce in many a life of lingering and exquisitely painful disease.

And now, having touched on all the points, by which you have very imperfectly presented my argument to the public, I beg leave here to add an observation or two upon a most remarkable statement in your final Report. Admissions, which you have there made, tell me how little reliance can be placed on your researches to detect lead in any water. The statement cannot but throw a shade of doubt on all your experimentation, in the minds of those accustomed to laboratory practice. I allude to this, that after the most positive assurance in your first and second Reports, "that a gallon of Croton water, which had stood thirty-six hours in a pipe already in use some months in New York, upon concentration to the volume of half a gill, or to one two-hundredths of its volume gave no trace of lead, (and you admit, (*Final Report*, p. 48,) "it is not difficult to detect lead in 100,000 times its weight of pure water,") you now assert, "that this examination did not preclude its possible presence." (*Loc. cit.*)

Sir, what is the object of chemical analysis? Is it not to discover truth? In your descent into the well, while the cord yet remained unloosed, may you proceed only a part of the way down, and then return with the solemn announcement that Truth is not there; while it was your bounden duty to have dipped your "silver bowl" while yet unbroken, to the lowest depths; and thence have drawn her forth to light?

What a compliment have you paid Prof. Booth! After parading his researches as confirmatory of your own, you turn round, and say to him, months after your and his statement has had its effect, that he was mistaken as well as yourself, in not pushing analysis a step farther, when

quite a different result, as you now show, could have been ^{well} be-
What evidence have we that all your experimentation has not ^{been} ~~sub~~
half way?—that results did not lie still deeper, which, if disclosed
would have produced a Report different from that which has been given
to the public? Results which may have influenced a decision founded
on applying the imperfect experience derived from one class of waters, to
another wholly different? This decision, whether or not well founded,
appears to have been so completely dependent on the question of experi-
ence, that I think the sons of science may congratulate themselves that
she has had no influence in its determination.

For your own reputation, for the honor of that Scientific School over
the chemical department of which you have been called to preside, for
shielding your character from the suspicion of wanting those habits of
rigid investigation, which the public have a right to demand of him, who
is thus called to impart, not only accurate knowledge, but the mode of at-
taining that by exact experiment and minute observation, I trust, my dear
Sir, that the statement to which I have alluded, will be pondered by you,
with a deep feeling of the effect it may have on that influence, which may
attach to any future experiments you may make for the public weal.

While I cannot but regret that the cause which gave rise to this state-
ment, should have ever existed, I rejoice to see in the avowal of your
error a germ of candor, on the stock of which may be hereafter ingrafted
that cautious induction which is the soul of true science.

The only point of defence of the safety of lead upon which you relied,
from your own experiments, having been proved untenable, and practically
abandoned by yourself, you have now set up a new theory. I will merely
remark, that, so far as your experimentation has been disclosed, or chemi-
cal deductions drawn by you from that of others, the theory and defence
are equally unsound.

Reserving to myself the liberty of making farther remarks on this point,
and on that "contribution to science," which you have intimated it is
your intention to make from your researches on the lead pipe question,

I am,

Your obedient servant,

SAMUEL L. DANA.

LOWELL, SEPT. 16, 1848.